Fractional Factorial Analysis for Flexible Pavement Performance Data

ROBERT C. HAIN and PAUL E. IRICK
Respectively, Assistant Chief and Chief of the Data Analysis Branch,
AASHO Road Test

• The first objective of the AASHO Road Test was to find significant relationships between pavement performance and certain characteristics of pavement design and applied loads. The rationale for achieving this objective has been described in considerable detail by Irick et al. (1). An understanding of this reference would be helpful since essentially the same rationale was used in the analysis described in this paper. The pavement serviceability-performance concept which served to define a performance variable was described by Carey and Irick (2). Statistical concepts primarily oriented toward the Road Test were explained elsewhere (3, 4, 5).

Within each Test Road loop the main flexible experiment (Design 1) sections existed as a complete factorial experiment; that is, within a loop every design factor level occurred in combination with all other design factor levels. In addition, the factorial experiment was designed so that a properly selected subset of all the sections would be analyzed independently of the remaining sections. This subset was called a fractional replicate; at the Road Test it was also called a factorial block. It is the purpose of this paper to show that an analysis using a subset of the flexible sections would have produced essentially the same results as an analysis of the complete factorial experiment (6).

It should be emphasized that certain assumptions must be true before a subset or fractional replicate of a complete factorial experiment can give useful information. Therefore, it may be necessary to employ pilot experiments to determine if these assumptions are true or false. At the Road Test these assumptions turned out to be valid for certain variables including performance. It then follows that for other experiments where these assumptions are true the fractionally replicated experiment has an economic advantage over a complete factorial experiment since it is smaller and can give equivalent information.

A secondary purpose of this paper is to describe fractional replicate concepts. The conditions which must be met before fractions of complete factorial experiments can be used are explained. There are many books dealing with fractional replicate theory; for further reference (7 through 12).

A simple example will be used to explain the statistical and mathematical concepts of fractional replication. For this example, the design variables are asphalt surface thickness and crushed stone base thickness; the asphalt is 3 and 5 in. thick and the crushed stone 3 and 9 in. thick. The dependent variable \( Y \) can be any measured variable: performance (properly defined), deflection or rut depth, for example. For illustration, the dependent variable is the performance of a section under a specified load, the same load for all sections.

STATISTICAL CONCEPTS

The effect of a design variable, such as surface thickness, may be thought of as a change in the measured variable (performance) which is clearly attributable to a change in the design variable. If the principle of balance is used, the effect of surface thickness may be estimated independently of the effect of base thickness. Conventional application of this principle leads to a complete factorial experiment, a "strong" balanced experiment. The strength of a factorial experiment is due, in part, to the following:

1. The main effect of each design factor may be estimated independently of the main effect of every other design factor.
2. Interaction of the design factors can be examined. (Briefly an interaction occurs when the effect of one variable is different as the levels of another variable are changed.)
3. Effects can be determined with relatively high precision and efficiency since all of the measured \( Y \)'s (dependent variables) are used to estimate each effect.
4. If certain effects are small or not significant they may be used to estimate error; that is, the effect of uncontrolled variation. This is sometimes referred to as the "hidden replication" present in a factorial experiment.

A factorial layout of the illustrative experiment is shown in Table 1. Balance is achieved by having 3 and 9 in. of base under both 3 and
by repeating the entire experiment. The
same or all of the sections more than once, or
able such as load applications), by building
or loop. The illustrative experiment could be
were the loops. Therefore, there was no direct
estimate of error for gaging the effect of lane
measurements were made on every section. Thus, at any level of
be repeated, or replicate measurements can be
variable. Replication makes it possible to esti-
how much increased performance was due to increasing surface thickness and how much was due to changing from poor to
good soil. The randomized order of the illustra-
tive sections is shown in Figure 1.
While it is never desirable that effects of un-
controlled variables be confused or confounded
with effects of controlled variables, there may
be advantages to purposeful confounding of
certain effects of controlled variables with one
another. In general, it is most useful to con-
confounding effects of controlled vari-les. It will be shown that this type of con-
found interacting effects of controlled vari-
able. It is most useful for reducing the size
of an experiment or for gaining information
about more variables.
One of the penalties of confounding “main
effects”, e.g., the surface effect and the base
effect, is that it is impossible to separate the
effects of the different variables. For example,
an experiment might be designed so that the
thinnest section had 0 in. of surface and 0 in.
of base and the next thickest section had 1 in.
of surface and 3 in. of base with each thicker
section having 1 in. more of surface and, at the
same time, 3 in. more of base. It would be im-
possible to tell how much change in a dependent
variable was associated with the 1-in. increase
in surface and how much was associated with
the 3-in. increase in base. There is, obviously,
no information about interacting effects. If it
can be assumed that an effect is negligible or is
not present, this effect can be confounded with
a second effect with no loss of information
about the second effect. In summary, confound-
ing, if it is properly done, may be beneficial;
Improperly done, confounding may make it
impossible to discriminate between the effects
of variables.
Confounding may be used for one of two
purposes:
1. To include more variables without chang-
ing the size of an experiment; the number of
sections will remain the same but the effects of
more variables will be examined.
2. To reduce the size of an experiment.

<table>
<thead>
<tr>
<th>Base Thickness (in.)</th>
<th>Surface Thickness (in.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>3</td>
<td>Y_{11}</td>
</tr>
<tr>
<td>9</td>
<td>Y_{21}, Y_{22}</td>
</tr>
</tbody>
</table>

5 in. of surface. Thus, this experiment has
2 \times 2 = 4 sections.
Let \( Y_{ij} \) be the performance of the section at
the \( i^{th} \) level of surface thickness and \( j^{th} \) level
of base thickness, where \( i = 1, 2 \) and \( j = 1, 2 \).
For example, \( Y_{21} \) denotes the performance of
the section with 5 in. of surface \((i = 2)\) and 3
in. of base \((j = 1)\).
The effect of base under 3 in. of surface can be
measured by observing the difference be-
tween \( Y_{11} \) and \( Y_{12} \) \((Y_{12} - Y_{11})\); the effect of
base can also be measured under 5 in. of sur-
face by observing the difference between \( Y_{21} \)
and \( Y_{22} \) \((Y_{22} - Y_{21})\). An interaction implies
that these differences are not the same, i.e.,
\( Y_{12} - Y_{11} \) is not equal to \( Y_{22} - Y_{21} \).
Before any statement can be made about the
significance of a surface thickness effect, for
example, it must be known how differently two
sections with the same surface thickness per-
form. That is, how do uncontrolled variables
affect performance. In general, the same de-
signs must be repeated or replicated in space
and/or time if any conclusion is to be made
about the significance of the effect of a design
variable. Replication makes it possible to esti-
the effects of uncontrolled variables on
the dependent variables. Replicates can be con-
structed in a variety of ways. The entire ex-
periment can be constructed at another time or
place, one or more of the original sections can
be repeated, or replicate measurements can be
made on the same section. Thus, at any level of
observation replication must be used if there is
any reason to draw conclusions about the
effects of controlled variables at that level.
Replication variation or replication error
serves as a yardstick against which the ap-
parent effects of controlled variables can be
gaged.
At the Road Test certain sections were repli-
cated in every loop and at times replicate
measurements were made on every section.
However, the lanes were not replicated nor
were the loops. Therefore, there was no direct
estimate of error for gaging the effect of lane
or loop. The illustrative experiment could be
replicated by taking repeated measurements
(without changing the level of any other vari-
able such as load applications), by building
some or all of the sections more than once, or
by repeating the entire experiment. The
amount of replication would depend on whether
conclusions would be made about measurement
variables, section design variables or “loop”
variables.

## Table 1

<table>
<thead>
<tr>
<th>Base Thickness (in.)</th>
<th>Surface Thickness (in.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>3</td>
<td>( Y_{11} )</td>
</tr>
<tr>
<td>9</td>
<td>( Y_{21}, Y_{22} )</td>
</tr>
</tbody>
</table>

### Con founding

Randomization reduces the risk that the ef-
effects of controlled variables will be confused
with the effects of uncontrolled variables that
vary systematically. For example, if the il-
ustrative experiment was laid out so that both
of the 3-in. surface sections were on relatively
poor soil and both of the 5-in. surface sections
were on relatively good soil then the effect of
surface would be confused or confounded with
the effect of soil. It would not be possible to
determine how much increased performance
was due to increasing surface thickness and how
much was due to changing from poor to
good soil. The randomized order of the illustra-
tive sections is shown in Figure 1.

While it is never desirable that effects of un-
controlled variables be confused or confounded
with effects of controlled variables, there may
be advantages to purposeful confounding of
certain effects of controlled variables with one
another. In general, it is most useful to con-
confounding effects of controlled vari-
able. It will be shown that this type of con-
found interacting effects of controlled vari-
able. It is most useful for reducing the size
of an experiment or for gaining information
about more variables.

One of the penalties of confounding “main
effects”, e.g., the surface effect and the base
effect, is that it is impossible to separate the
effects of the different variables. For example,
an experiment might be designed so that the
thinnest section had 0 in. of surface and 0 in.
of base and the next thickest section had 1 in.
of surface and 3 in. of base with each thicker
section having 1 in. more of surface and, at the
same time, 3 in. more of base. It would be im-
possible to tell how much change in a dependent
variable was associated with the 1-in. increase
in surface and how much was associated with
the 3-in. increase in base. There is, obviously,
no information about interacting effects. If it
can be assumed that an effect is negligible or is
not present, this effect can be confounded with
a second effect with no loss of information
about the second effect. In summary, confound-
ing, if it is properly done, may be beneficial;
Improperly done, confounding may make it
impossible to discriminate between the effects
of variables.
through the use of fractional replication; the number of factors remains the same but there are fewer sections than in a complete factorial experiment.

The example will be used to show how confounding may be applied in a statistical sense to satisfy either of the objectives. First of all for the illustrative experiment, let $D_i$ stand for the surface variable and let $D_{ij}$ stand for the $i^{th}$ level of this variable where $i = 1, 2$. Similarly let $D_{jk}$ stand for the $j^{th}$ level of the base variable where $j = 1, 2$. For convenience $D_i$ and $D_j$ are coded so that the thinner level is coded to $-1$ and the thicker level is coded to $+1$. The coded variables are called $A$ and $B$. For example,

$$A_i = \frac{D_{1i} - 4}{1} = \frac{3 - 4}{1} = -1$$

and

$$A_2 = \frac{D_{2i} - 4}{1} = \frac{5 - 4}{1} = +1$$

Thus, Section 2 with an observed performance equal to $Y_{11}$ is coded as $(-1, -1)$. The interaction between surface and base is called $AB$ and is the product of $A_i$ times $B_j$. The coding is given in Table 2.

Before the experiment is performed however, another variable, subbase thickness, is included. This variable is denoted by $D_{jk}$, its levels are denoted by $D_{jk}$ where $k = 1, 2$. It must be assumed that there are no interactions between either surface and subbase or base and subbase. Furthermore, it is assumed that there is no interaction between surface and base, i.e., that the $AB$ interaction is negligible. $AB$ can be equated to or confounded with the new variable subbase in the following manner. Subbase

<table>
<thead>
<tr>
<th>Section</th>
<th>Observation</th>
<th>Surface Thickness (in.)</th>
<th>Base Thickness (in.)</th>
<th>Coded Levels*</th>
<th>Subbase Thickness (in.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$D_{1i}$</td>
<td>$D_{2i}$</td>
<td>$A_i$</td>
<td>$B_i$</td>
</tr>
<tr>
<td>2</td>
<td>$Y_{11}$</td>
<td>3</td>
<td>3</td>
<td>$-1$</td>
<td>$-1$</td>
</tr>
<tr>
<td>1</td>
<td>$Y_{12}$</td>
<td>3</td>
<td>3</td>
<td>$-1$</td>
<td>$+1$</td>
</tr>
<tr>
<td>4</td>
<td>$Y_{21}$</td>
<td>5</td>
<td>3</td>
<td>$+1$</td>
<td>$-1$</td>
</tr>
<tr>
<td>3</td>
<td>$Y_{22}$</td>
<td>5</td>
<td>9</td>
<td>$+1$</td>
<td>$+1$</td>
</tr>
</tbody>
</table>

$*A_i = \frac{D_{1i} - 4}{1}, B_i = \frac{D_{2i} - 6}{3}, C_k = \frac{D_{jk} - 6}{2}$
PAVEMENT PERFORMANCE

Figure 2. Randomized order of illustrative sections including subbase thickness, Block 1.

TABLE 3
ILLUSTRATIVE EXPERIMENT, BLOCK 2

<table>
<thead>
<tr>
<th>Section</th>
<th>Observation</th>
<th>Surface Thickness (in.)</th>
<th>Surface Thickness (in.)</th>
<th>Coded Levels*</th>
<th>Subbase Thickness (in.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$D_{1}$</td>
<td>$D_{2}$</td>
<td></td>
<td>$D_{3}$</td>
</tr>
<tr>
<td>8</td>
<td>$Y'$</td>
<td>3</td>
<td>3</td>
<td>-1</td>
<td>-1</td>
</tr>
<tr>
<td>6</td>
<td>$Y'$</td>
<td>3</td>
<td>9</td>
<td>-1</td>
<td>+1</td>
</tr>
<tr>
<td>5</td>
<td>$Y'$</td>
<td>5</td>
<td>3</td>
<td>+1</td>
<td>+1</td>
</tr>
<tr>
<td>7</td>
<td>$Y'$</td>
<td>9</td>
<td>9</td>
<td>-1</td>
<td>-1</td>
</tr>
</tbody>
</table>

thickness ($D_{3}$) is coded to $-1$ for the thinner level and to $+1$ for the thicker level. The coded levels are denoted by $C_{k}$. For example,

\[
C_{1} = \frac{D_{31} - 6}{2} = \frac{-6}{2} = -1.
\]

Then $C = -1$ can be equated to $AB = -1$ and $C = +1$ can be equated to $AB = +1$. This technique results in 8 in. of subbase being confused with $AB = +1$ while 4 in. of subbase is confused with $AB = -1$ (Table 2). In other words, subbase thickness has been confused with the surface-base $(AB)$ interaction. The result is that another factor has been included without increasing the size of the original experiment. It must be emphasized again that any interacting effects between the variables must be negligible.

The foregoing explanation showed that with proper design an extra factor could be included in an experiment without increasing the size of the experiment. Using the same assumptions of no interactions a subset or a fractional replicate of a complete factorial experiment could also be constructed. It will be shown that the experimental design in Table 2 can also be viewed as a one-half replicate of a complete factorial experiment. The complete factorial experiment would have three factors at two levels and contain eight sections. Let the experiment shown in Table 2 be called Block 1 to imply that it is a fractional replicate. The randomized layout of this experiment (Block 1) is shown in Figure 2.

Table 3 shows the design of an experiment much like that in Table 2. However, there are four new sections called 5, 6, 7 and 8 with observations on the dependent variable (performance) denoted by $Y'$. As before, the 3-in.
Figure 3. Randomized order of illustrative sections including subbase thickness, Block 2.

**TABLE 4**

<table>
<thead>
<tr>
<th>Section</th>
<th>Observation</th>
<th>Surface Thickness (in.)</th>
<th>Base Thickness (in.)</th>
<th>Subbase Thickness (in.)</th>
<th>Coded Levels*</th>
<th>Three Factor Interaction</th>
<th>Block</th>
</tr>
</thead>
<tbody>
<tr>
<td>8</td>
<td>Y'11</td>
<td>3</td>
<td>3</td>
<td>4</td>
<td>-1</td>
<td>-1</td>
<td>-1</td>
</tr>
<tr>
<td>2</td>
<td>Y'1</td>
<td>3</td>
<td>3</td>
<td>8</td>
<td>-1</td>
<td>-1</td>
<td>+1</td>
</tr>
<tr>
<td>1</td>
<td>Y'12</td>
<td>3</td>
<td>9</td>
<td>8</td>
<td>-1</td>
<td>+1</td>
<td>1</td>
</tr>
<tr>
<td>6</td>
<td>Y'y2</td>
<td>3</td>
<td>9</td>
<td>8</td>
<td>-1</td>
<td>-1</td>
<td>+1</td>
</tr>
<tr>
<td>4</td>
<td>Y'y1</td>
<td>5</td>
<td>3</td>
<td>4</td>
<td>+1</td>
<td>-1</td>
<td>-1</td>
</tr>
<tr>
<td>5</td>
<td>Y'y12</td>
<td>5</td>
<td>3</td>
<td>8</td>
<td>+1</td>
<td>-1</td>
<td>-1</td>
</tr>
<tr>
<td>7</td>
<td>Y'y22</td>
<td>5</td>
<td>9</td>
<td>4</td>
<td>+1</td>
<td>+1</td>
<td>2</td>
</tr>
<tr>
<td>3</td>
<td>Y'y11</td>
<td>5</td>
<td>9</td>
<td>8</td>
<td>+1</td>
<td>+1</td>
<td>1</td>
</tr>
</tbody>
</table>

*$_{A} = \frac{D_{h} - 6}{1}$,  $B = \frac{D_{h} - 6}{3}$,  $C = \frac{D_{h} - 6}{2}$

level of surface has been coded to $-1$ and the 5-in. level has been coded to $+1$; the 3-in. level of base has been coded to $-1$ while the 9-in. level has been coded to $+1$. Again, with the assumption of no interaction, a third variable, subbase, has been introduced. However, this time coded subbase ($C_k$) has been equated to or confounded with $-AB$. Table 3 indicates that 8 in. of subbase has been confounded with $AB = +1$, whereas 4 in. of subbase has been confounded with $AB = -1$. Subbase thickness has now been confounded in a slightly different manner than in Block 1 with the surface-base ($-AB$) interaction. This is another one-half replicate of an experiment with three factors at two levels. This one-half replicate will be called Block 2 (Fig. 3).

Blocks 1 and 2 are in a sense complementary. This can be seen by observing Section 2 in Block 1 (Table 2) and Section 8 in Block 2 (Table 3). Both sections have 3 in. of surface and 3 in. of base. However, under Section 2 the subbase is 8 in. thick, whereas under Section 8 it is 4 in. thick. Essentially the same is true for each pair of sections with equal surface and subbase levels; a section with 4 in. of subbase in one block will have 8 in. of subbase in the other block or vice-versa.

Table 4 shows the design level layout for an experiment with three factors, surface thick-
ness, base thickness, and subbase thickness at two levels. There are \(2 \times 2 \times 2 = 2^3 = 8\) experimental sections. The coded levels \(A_i\), \(B_i\), and \(C_i\) are calculated using exactly the same method described previously. There are also three 2-factor interactions denoted by \(AB\), \(AC\), and \(BC\); and a 3-factor interaction denoted by \(ABC\). Only the levels of the 3-factor interaction are shown. Four of the sections have exactly the same levels as the sections in Block 1 (Table 2); they are labeled in the last column of Table 4 as Block 1. In addition, each of these sections labeled Block 1 has \(+1\) for \(ABC\). The remaining sections have exactly the same levels as the sections in Block 2 (Table 3); they are labeled Block 2. Each of these sections has a \(-1\) for \(ABC\). Thus, Block 1 and Block 2 correspond to four sections selected from eight sections of a \(2^3\) complete factorial experiment. Therefore, the blocks are complementary to each other. This is the reason why each block is called a one-half replicate of \(2^3\) complete factorial experiment. Each block can be utilized as an independent experiment and analyzed for the effects of surface, base, and subbase (provided that there are no interactions). The complete factorial experiment would, of course, give complete information about the effects of the design variables and their interactions.

In summary, it has been demonstrated that with four sections and negligible interacting effects it is possible to test three factors at two levels. The first purpose was satisfied since it was desired to include an additional variable subbase without changing the size of the original \(2 \times 2\) experiment. A complete factorial experiment containing three factors at two levels would have eight sections. Full information on the effects of the three factors and their interactions would have been available. However, with the assumption that interactions were negligible it was possible to obtain information about the main effects of the three factors using only four sections. Obviously no information is available about interactions. This would satisfy the second purpose since the size of the experiment was halved and, in general, four sections would cost less to test than eight sections.

It should be added that the illustrative experiment is impractical for a normal experiment for the following reasons:

1. There is no provision for estimating replication error—this could be remedied by replicating.
2. With only two levels for each variable there is no information available about curvature or trends; thus, it might be rash to fit an empirical model to the data.
3. With only four observations one extreme value or outlying point would have a drastic effect on the analysis.

This experiment might be of some practical value if there is a prior estimate of error or if it is only necessary to estimate the magnitude of an effect.

The preceding discussion about confounding was intended to show that if there is information about interactions it is possible to design an experiment which makes information available more economically than a complete factorial experiment. This might be done in one of two ways:

1. Retaining the same number of sections but putting additional factors into the experiment.
2. Studying the same number of factors but constructing fewer sections.

The same objective, that of studying the effects of variables and their interactions, could still be reached.

In addition, the fractional replicate has other advantages:

1. If the number of factors and/or levels of an experiment is large the number of sections needed for a complete factorial experiment may become prohibitive. There may not be enough time or money available to perform the complete factorial experiment.
2. Using a fraction of a complete factorial it is less disheartening and costly to include design variables which show up to have no effect on the dependent variable. Furthermore, if an experiment is part of a series of experiments the next experiment, where the non-significant factor will be dropped, will occur sooner.
3. If it is necessary to obtain more information about a set of effects and their interactions more of the sets of fractional replicates or even the complete factorial experiment can be performed.

**CONFOUNDING AT THE AASHO ROAD TEST**

The flexible tangent of each loop at the AASHO Road Test was designed as a complete factorial experiment. There were three pavement design factors, asphalt surface thickness convention (usually called surfacing thickness), crushed limestone base thickness and sand-gravel subbase thickness. Surface thickness convention implies that somewhat different construction procedures and materials were used for different thicknesses of surface.

The pavement design factors in Loops 3 to 6 (called the main traffic loops) had three levels. For example, in Loop 3 there were 2, 3 and 4 in. of surface, 0, 3 and 6 in. of base and 0, 4 and 8 in. of subbase. Thus, there were \(3 \times 3 \times 3 = 3^3 = 27\) structural sections in the flexible tangent of each main traffic loop.
Loop 2 had only two levels of subbase thickness, 0 and 4 in., so there were $3 \times 3 \times 2 = 18$ structural sections on the flexible side of Loop 2.

In addition, there was a load (or lane) factor in each of the traffic loops (2 to 6). For example, Loop 3 had 12-kip single-axle trucks in the inner lane and 24-kip tandem-axle trucks in the outer lane. In the main traffic loops all single-axle vehicles were in the inner lane and all tandem-axle vehicles were in the outer lane so that load type was confounded with lane. Loop 2 had single-axle vehicles with different loads in each lane.

The 27 structural sections in each of the main traffic loops were partitioned into three blocks. Each block was a one-third fractional replicate of the complete factorial experiment in that loop. (The fractional replicates were designed according to the layout in Table 9.62 (7).) This was not done in Loop 2. The fractional replicates were commonly called factorial blocks. (For brevity a block in this paper will denote a one-third fractional replicate of the complete factorial experiment in a loop.) Each block was assigned at random to one portion of the flexible side of a loop. The same randomization was used in all four loops. The randomized blocks were designated as the west factorial block, the center factorial block and the east factorial block; or alternatively as Block 1, Block 2 and Block 3. A typical layout of the randomized factorial blocks is shown in Figure 4. Structural sections were arranged in random order from west to east within their respective blocks. Figure 4 also shows a typical arrangement of structural sections in blocks. The section-in-block randomization was different for each loop and block. The remaining flexible sections in each loop were used in special studies of base type and shoulder paving. The special studies were also laid out in randomized blocks, there being four special study blocks in each main loop tangent.

The blocks or fractional replicates had some important characteristics designed into them; some of these characteristics which were useful for analyzing the results of the experiment were as follows:

1. All three levels of each of the pavement design factors occurred in each block.
2. The average total pavement thickness was the same for each block in a loop.
3. Three sections total pavement thickness were replicated or constructed twice in each loop; one of these sections was assigned to each block.
4. The total thickness of these replicate sec-
tions was equal to the average total thickness of the blocks, providing an arbitrary rule for deciding which designs to replicate.

It might be profitable to discuss the possibility of having confounded effects of variables at the Road Test. As has been pointed out confounding could have been used to advantage in one of two ways:

1. The size of the experiment could have been reduced by including only one or two of the three fractional replicates (blocks).
2. More variables could have been introduced with the same number of sections.

The possibility of confounding and thereby reducing the size of the experiment or including more variables was carefully considered prior to designing the Test Road. However, as was shown in the illustrative example, certain interactions between surface, base and subbase had to be negligible before a confounded design could provide useful information. Because it was felt that there was insufficient information about interactions to warrant confounding at the Road Test, the complete factorial experiment was constructed in each loop. To provide additional useful information about future possibilities for using confounding, the sections within the complete factorial experiment were partitioned into fractional replicates or blocks. It is the purpose of this paper to show that since the complete factorial experiment was used and interactions were virtually negligible, any fractional replicate should give essentially the same information as the complete factorial experiment. Inasmuch as the complete factorials were available, it was possible to compare the results from each fractional replicate with each other and with the complete factorial experiment.

**PAVEMENT PERFORMANCE**

The same pavement performance data were used as were previously reported (5). In addition, the following definitions have been given (1):

- **Serviceability trend** is the plot of smoothed serviceability history values \( p \) on an accumulated axle applications scale \( W \) where axle applications may be weighted or unweighted; and
- **Performance** of a pavement is given by its serviceability trend.

For the purposes of this paper flexible weighted applications only were used. It was supposed that unweighted applications would result in essentially the same conclusions. Every section was represented by five pairs of coordinates from its serviceability trend. Sections whose serviceability trend had reached 1.5 were represented by five values of \( W \) noted when \( p \) reached 3.5, 3.0, 2.5, 2.0 and 1.5. Sections whose trend had not reached 1.5 were represented by five values of \( p \) recorded on 11, 22, 33, 44 and 55 index days. For analytical purposes it was more convenient to use log \( W \) instead of \( W \); therefore, a section which reached 1.5 after one million weighted applications of load would have as one of its coordinates 6.0 and 1.5 for log \( W \) and \( p \), respectively.

**ANALYSIS**

The procedures used in the analysis described in this paper were essentially the same as those previously described (1). The procedures were used to fit an equation to the observed performance data so that the equation related performance to pavement design and load variables. Certain constants were specified; others were estimated using statistical and mathematical techniques. The analysis described in this paper differed from previous analyses in that each factorial block was treated as a separate entity. The results from the blocks were compared among themselves and with the results from an analysis of the complete factorial experiment.

The results of this analysis cannot prove that the procedures used will always result in unbiased estimates of the coefficients; the results can only show that when the procedures are used with slightly different sets of data the results do not change very much.

The performance equation used at the AASHO Road Test was of the form:

\[
p = c_0 - (c_0 - c_1) \left( \frac{W}{\rho} \right)^\beta
\]

in which \( c_1 \leq p \leq c_0 \).

This equation was transformed so that the resulting equation was linear in \( \beta \) and log \( \rho \)

\[
\log \left( \frac{c_0 - p}{c_0 - c_1} \right) = \beta \left( \log W - \log \rho \right)
\]

or

\[
\log \left( \frac{c_0 - p}{c_0 - c_1} \right) = \beta \left( \log W - \log \rho \right)
\]

For convenience the left side of Eq. 3 was called \( G \), so that

\[
G = \log \left( \frac{c_0 - p}{c_0 - c_1} \right)
\]

in which \( G \) is undefined unless \( p < c_0 \). Substituting into Eq. 3 from Eq. 4

\[
G = \beta \left( \log W - \log \rho \right)
\]

For the flexible pavement analysis \( c_0 \) was 4.2, the average initial serviceability of all flexible factorial sections, while \( c_1 \) was 1.5. The average initial serviceability of all the sections in a block...
was also computed for each block. These values either rounded to 4.2 or were not significantly different from 4.2; therefore, $c_0 = 4.2$ was used for all blocks. The $p$ and $W$ coordinates for each section were converted to $G$ and log $W$ coordinates. In general, there were five new coordinates, however, if any $p$'s were greater than 4.2 there were correspondingly fewer points. A least squares linear regression analysis was used to fit a straight line to these points. The slope of the line, $\beta$, was estimated by $\beta$; the intercept on the abscissa, log $p$, was estimated by log $\beta$. The equations for $\beta$ and log $\beta$ are shown in Figure 5, box 2.

It was not possible in this analysis to use Loop 2 as it was designed since there were only two levels of subbase (0 and 4 in.) and no factorial blocks as such. However, for the block analysis hypothetical values of $\beta$ and log $\beta$ were inserted for fictitious sections with 8 in. of subbase. These hypothetical values were based on experience at the Road Test indicating that 4 in. of subbase were approximately equal to 3 in. of base. This assumed, for example, an
Figure 5. Flow chart (continued).
approximately equivalent relationship between a real section of 2-3-4 design for surface, base and subbase, respectively, and a section of 2-0-8 design. Removing 3 in. of base could be compensated for by adding 4 in. of subbase. This does not imply that this relationship holds outside of the Road Test, for other combinations of base and subbase material or for analyses of other data. It should be noted that the effect of this assumption on the analysis was relatively

Figure 5. Flow chart (continued).
small since, in general, Loop 2, lane 1 was not included in the performance analysis and the nine fictitious sections in Loop 2, lane 2 were among 234 observed sections.

It was assumed that \( \beta \) was related to design and load in the following manner,

\[
\beta = \beta_o + \frac{B_o (L_1 + L_2)^{b_1}}{(b_1D_1 + b_2D_2 + b_3D_3 + b_4)^{n_1}L_2^{n_2}} \tag{6}
\]

in which

\( \beta_o \) = an assumed minimum value for \( \beta \);
\( L_1 \) = the nominal axle load, in kips;
\( L_2 \) = surface thickness in inches;
\( D_1 \) = base thickness, in inches;
\( D_2 \) = subbase thickness, in inches; and
\( b_1 \) = a positive constant with an assumed value.

The remaining letters denote positive constants which were estimated from the \( \beta \) values. Rearranging and taking logarithms, Eq. 6 reduced to

\[
\log (\beta - \beta_o) = \log B_o - B_1 \log (b_1D_1 + b_2D_2 + b_3D_3 + b_4) + B_2 \log (L_1 + L_2) - B_3 \log L_2 \tag{7}
\]

At the Road Test it was assumed that \( \beta_o = 0.4 \) and \( b_4 = 1.0 \). Eq. 7 reduced to

\[
\log (\beta - 0.4) = \log B_o - B_1 \log (b_1D_1 + b_2D_2 + b_3D_3 + 1) + B_2 \log (L_1 + L_2) - B_3 \log L_2 \tag{8}
\]

This equation was a linear regression equation with \( \log (\beta - 0.4) \) as the dependent variable and three independent variables: (a) \( \log (b_1D_1 + b_2D_2 + b_3D_3 + 1) \); (b) \( \log (L_1 + L_2) \); and (c) \( \log L_2 \).

The parameters were estimated by an iterative process. The flow chart for this process is shown in Figure 5. Each of the three blocks were analyzed independently using this iterative process. In other words, three parallel block analyses were conducted simultaneously following the procedure in the flow chart.

First of all (as shown in Box 3), regression analyses were made for each of nine lanes using \( \log (\beta - 0.4) \) as the dependent variable and \( \log (0.4D_1 + 0.14D_2 + 0.11D_3 + 1) \) as the independent variable. Loop 2, lane 1 was omitted since very few of the \( \beta \) values were greater than the minimum of 0.4, i.e., not enough sections showed a decline in serviceability. The estimates of \( \log B_o \) were ignored; the nine lane estimates of \( B_1 \) were formed into a weighted average (Box 4) to give an estimate of \( B_1 \) called \( B_1 \). Table 5 shows the estimate of \( B_1 \) for the necessary iterations as well as for the estimates derived from the complete factorial experiment (\( \beta \)).

For simplicity let \( D = b_1D_1 + b_2D_2 + b_3D_3 \). A transposition of \( B_1 \log (D + 1) \) in Eq. 8 yielded

\[
\log (\beta - 0.4) + B_1 \log (D + 1) = \log B_o + B_2 \log (L_1 + L_2) - B_3 \log L_2 \tag{9}
\]

The left side of Eq. 9 was called an adjusted \( \log (\beta - 0.4) \). The lane average of the left

<table>
<thead>
<tr>
<th>Reference (( \theta ))</th>
<th>Over-all Equation</th>
<th>( \beta_o )</th>
<th>( B_o )</th>
<th>( B_1 )</th>
<th>( b_1 )</th>
<th>( b_2 )</th>
<th>( b_3 )</th>
<th>( b_4 )</th>
<th>( B_2 )</th>
<th>( B_3 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Initial Values</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block 1</td>
<td>0.4</td>
<td>0.081</td>
<td>5.19</td>
<td>0.44</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.23</td>
<td>3.23</td>
<td></td>
</tr>
<tr>
<td>First Iteration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block 2</td>
<td>0.4</td>
<td>0.138</td>
<td>4.15</td>
<td>0.44</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.07</td>
<td>3.07</td>
<td></td>
</tr>
<tr>
<td>Block 3</td>
<td>0.4</td>
<td>0.083</td>
<td>5.30</td>
<td>0.44</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.56</td>
<td>3.56</td>
<td></td>
</tr>
<tr>
<td>Second Iteration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block 1</td>
<td>0.4</td>
<td>0.065</td>
<td>5.97</td>
<td>0.44</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.62</td>
<td>3.62</td>
<td></td>
</tr>
<tr>
<td>Block 2</td>
<td>0.4</td>
<td>0.151</td>
<td>4.15</td>
<td>0.46</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.54</td>
<td>3.54</td>
<td></td>
</tr>
<tr>
<td>Block 3</td>
<td>0.4</td>
<td>0.085</td>
<td>5.90</td>
<td>0.45</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>3.63</td>
<td>3.63</td>
<td></td>
</tr>
<tr>
<td>Final Values</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block 1</td>
<td>0.4</td>
<td>0.063</td>
<td>5.90</td>
<td>0.45</td>
<td>0.12</td>
<td>0.11</td>
<td>1.0</td>
<td>3.63</td>
<td>3.63</td>
<td></td>
</tr>
<tr>
<td>Block 2</td>
<td>0.4</td>
<td>0.151</td>
<td>4.18</td>
<td>0.46</td>
<td>0.15</td>
<td>0.10</td>
<td>1.0</td>
<td>2.54</td>
<td>2.54</td>
<td></td>
</tr>
<tr>
<td>Block 3</td>
<td>0.4</td>
<td>0.085</td>
<td>5.83</td>
<td>0.42</td>
<td>0.16</td>
<td>0.12</td>
<td>1.0</td>
<td>3.61</td>
<td>3.61</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Reference (( \theta ))</th>
<th>Over-all Equation</th>
<th>( \log A_o )</th>
<th>( A_1 )</th>
<th>( a_1 )</th>
<th>( a_2 )</th>
<th>( a_3 )</th>
<th>( a_4 )</th>
<th>( A_2 )</th>
<th>( A_4 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Block Analysis</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block 1</td>
<td>5.96</td>
<td>9.36</td>
<td>0.44</td>
<td>0.14</td>
<td>0.11</td>
<td>1.0</td>
<td>4.79</td>
<td>4.33</td>
<td></td>
</tr>
<tr>
<td>Block 2</td>
<td>5.72</td>
<td>9.12</td>
<td>0.45</td>
<td>0.15</td>
<td>0.10</td>
<td>1.0</td>
<td>4.50</td>
<td>4.00</td>
<td></td>
</tr>
<tr>
<td>Block 3</td>
<td>6.06</td>
<td>9.40</td>
<td>0.42</td>
<td>0.16</td>
<td>0.11</td>
<td>1.0</td>
<td>4.99</td>
<td>4.43</td>
<td></td>
</tr>
</tbody>
</table>
side of Eq. 9 was called an adjusted lane mean; it was also a linear function of log \((L_1 + L_2)\) and log \(L_i\). Next in the procedure, average \([\log (\beta - 0.4)] + \bar{B}\) average \([\log (D + 1)]\) was regressed on \(\log (L_1 + L_2)\) and \(\log L_i\) to obtain estimates of \(B_n, B_2\) and \(B_3\) (Box 5, Fig. 5). The estimates for \(B_n, B_2\) and \(B_3\) are also shown in Table 5. For each block there was an equation which related \(\beta\) to the specified design and load variables. The equation derived at the Road Test from the complete factorial experiment was the following (6):

\[
\log (\beta - 0.4) = -1.09 - 5.19 \log (0.44D_1 + 0.14D_2 + 0.11D_3 + 1) + 3.23 \\
\log (L_1 + L_2) - 3.23 \log L_i
\]

or

\[
\bar{\beta} = 0.4 + 0.081 (L_1 + L_2)^{1.22} \\
(0.44D_1 + 0.14D_2 + 0.11D_3 + 1)^{0.19} L_i^{0.33}
\]

where \(\bar{\beta}\) was a final estimate of \(\beta\).

The next step in the procedure was to rewrite Eq. 5 so that \(\log p\) became a function of \(\beta\):

\[
\log p = \log W - \frac{G}{\beta}
\]

\(\bar{\beta}\) was introduced as an estimate of \(\beta\) and a new, second estimate of \(\log p\) was computed from

\[
\log p = \log W - \frac{G}{\bar{\beta}}
\]

Box 6, Figure 5 shows the computations.

New estimates of \(b_n, b_2\) and \(b_3\) were then found by making an analysis of variance in each loop using \(\log \bar{\beta}\) as the dependent variable and \(D_1, D_2\) and \(D_3\) as the independent variables (Box 7, Fig. 5). This analysis assumed a linear relationship between \(\log p\) and \(D_1, D_2\) and \(D_3\). This was done only to provide estimates of \(b_n, b_2\) and \(b_3\) and was not hypothesized as a model. The new estimates of \(b_n, b_2\) and \(b_3\) were averages of the coefficients computed from the Loop 3, 4 and 5 analyses. This was done because the estimates from Loops 3, 4 and 5 always tended to be homogeneous, whereas those from Loop 2 were high and those from Loop 6 were low.

The new estimates of \(b_n, b_2\) and \(b_3\) were compared to those found in the previous iteration (Table 5). If they were equal the iteration was stopped. If the coefficients were different the new estimates of \(b_n, b_2\) and \(b_3\) were used in \(\log (D + 1)\) and the complete procedure of estimating \(\beta\) was repeated starting with Eq. 8 (Box 3). The estimates of the various parameters computed during successive iterations are also shown in Table 5.

By the third iteration the input values of \(b_n, b_2\) and \(b_3\) (2nd iteration) and the output of the analysis of variance (3rd iteration) were equal when rounded to two decimal places. Iteration was stopped and the final procedure for estimating \(\log p\) was started.

The equation relating \(\log p\) to design and load variables was assumed to have the form

\[
\rho = \frac{A_0 (a_1D_1 + a_2D_2 + a_3D_3 + a_4) + L_i^{1.1}}{(L_1 + L_2)^{0.1}}
\]

in which the variables and parameters had the same properties as specified for Eq. 6. In addition, it was assumed that \(b_n = a_n, b_2 = a_2\) and \(b_3 = a_3\). This was the reason for the iterative procedure.

Logarithms of both sides of Eq. 14 were taken to produce the following multiple regression equation:

\[
\log p = \log A_0 + \log (D + a_1) - A_2 \\
\log (L_1 + L_2) + A_3 \log L_i
\]

or

\[
\log p = A_0 + \log (D + a_1) - A_2 \\
\log (L_1 + L_2) + A_3 \log L_i
\]

in which \(D = a_1D_1 + a_2D_2 + a_3D_3 = b_1D_1 + b_2D_2 + b_3D_3\) and \(a_1 = 1\).

The log \(\bar{\beta}\) data obtained in Eq. 13 (Box 6, Fig. 5) were regressed on \(\log (D + 1)\) to obtain estimates of \(A_1\) for each lane (Box 10). Loop 2, lane 1 was included since the log \(\bar{\beta}\) data for this lane were quite reasonable. The regression slopes \(A_1\) were averaged (Box 11) for the ten lanes to give an estimate, \(A_1\), for each block. Table 5 shows these estimates.

A transposition of \(A_1, \log (D + 1)\) in Eq. 15 then yielded

\[
\log p - A_1, \log (D + 1) = \log A_0 - \\
A_2 \log (L_1 + L_2) + A_3 \log L_i
\]

The left side of Eq. 16 was called an adjusted log \(\rho\). The lane average of the left side of Eq. 16 was called an adjusted lane mean; as for \(\log (\beta - 0.4)\) it was a linear function of log \((L_1 + L_2)\) and log \(L_i\). The lane average was denoted by average log \(p\). Next, average [ log \(\rho\) - \(A_1\) average [log \((D + 1)\)] was regressed on \(\log (L_1 + L_2)\) and \(\log L_i\) (Box 12), yielding estimates of \(A_n, A_1\) and \(A_i\). For each block there was now an equation which related log \(\rho\) to the specified design and load variables. The equation derived from the complete factorial analysis at the Road Test was (6)

\[
\log \bar{\beta} = 5.93 + 9.36 \log (0.44D_1 + 0.14D_2 + 0.11D_3 + 1) - 4.79 \log (L_1 + L_2) + 4.33 \log L_i
\]

or

\[
\bar{\beta} = \frac{10^{5.93} (0.44D_1 + 0.14D_2 + 0.11D_3 + 1)^{0.36} L_i^{0.33}}{(L_1 + L_2)^{0.70}}
\]
in which log $p$ was the final estimate of log $p$. The corresponding parameter estimates for each block are shown in Table 5.

COMPARISON OF THE ANALYSES

It would be extremely difficult to construct a test which could tell if $a_1$, $a_2$, or $a_3$ were significantly different between blocks. However, a mere scanning of these coefficients seemed to indicate that they were homogeneous. In addition, the average of $a_1$, $a_2$, and $a_3$ across blocks was 0.44, 0.14 and 0.11, respectively, the same as the values from the analysis of the complete factorial experiment. This was an indication that this procedure would give consistent results when applied to data that were somewhat different.

Tests were made to determine if the other coefficients, such as log $B_1$, $B_2$, and $B_3$ were significantly different between blocks. An exact significance test would necessitate an explicit formula for the variance of the estimated coefficients, an extremely difficult task for this procedure. Statistical "t" tests were used which indicated that the coefficients $B_1$, $B_2$, $A_1$, and $A_2$ were significantly different for Block 2 compared to Blocks 1 or 3. Because of the strong relationship between the procedures for estimating log $(\beta - \beta_i)$ and log $p$ it was difficult to reduce what these significant differences implied. However, these differences had little effect on the practical outcome of the experiment.

In HRB Special Report 61E, data from the complete factorial experiment were substituted into the performance equation in order to determine residuals from the over-all equation. The summary of these residuals is given in Table 5. In order to compare the goodness of fit of the block equations with the over-all equation, it was decided to compute residuals for all factorial section data, using first the performance equation derived only from Block 1 sections, then Block 2 sections, and finally from Block 3 sections. The following equations were used to obtain estimates for log $W$ and for $p$.

$$\hat{p} = 4.2 - 2.7\left(\frac{W}{p}\right)$$  \hspace{1cm} (19)

$$\log W = \log \hat{p} + \frac{G}{\beta}$$  \hspace{1cm} (20)

Residuals were computed according to the equations:

$$\Delta p = p - \hat{p}$$  \hspace{1cm} (21)

$$\Delta \log W = \log W - \log \hat{W}$$  \hspace{1cm} (22)

The mean absolute residuals for all sections are shown in Table 6 for both the equations derived from the complete factorial experiment and each of the three factorial blocks. Correlation indexes are also shown for the log $W$ estimates in each of the four cases.

Generally the loop-lane-block residuals were of the same size as the loop-lane residuals for the complete factorial experiment. There was no apparent trend for any one block to have smaller residuals than those for the complete factorial experiment.

If residuals from block equations are computed for only those sections that were in the respective blocks, then the residuals average to be somewhat smaller than for the over-all equations. However, all sections have been included in the residual summary of Table 6 to show that the equation from any one block can be used to predict the performance of sections whose designs did not appear in the block at all—and that the precision of prediction is essentially as though complete factorial data had been used to determine the equation.

Figures 22 and 23 (6) show graphically the results of fitting Eq. 1 to the observed data. The relationships between design and axle load applications for various loads are illustrated at $p = 2.5$ and 1.5. As was stated in AASHO Road Test Report 5, "These relationships are not intended to be design equations. However, they can serve as a basis for design procedures in which variables not included in the Road Test, such as soil type, are considered." The corresponding relationships can be compared for a one-third fractional replicate and the complete factorial experiment. Figure 6 shows a comparison for an 18-kip single-axle load.

There is obviously little practical difference between the results from a one-third fractional replicate and the complete factorial experiment. In the range of weighted axle-load applications shown, the maximum discrepancy between the complete factorial analysis (6) and a factorial block analysis for a constant number of axle-load applications corresponds to an approximate thickness index of 0.2. This roughly corresponds to 1/2 in. of surface, 1 1/2 in. of base or 2 in. of subbase. However, this variation is within the plausible amount of variation when other factors such as soil, construction, climate, methodology for analysis, and location are varied. In other words, there are many more variables which would affect the final estimates if they were included. There is no reason to believe that the variation between block estimates is any greater than the variation attributable to other omitted factors. From this reasoning the block analyses can be said to give acceptable and valid performance equations relative to the equations determined by data from the complete factorial experiment.

CONCLUSIONS

1. The performance analysis could have been made with a properly selected one-third fractional replicate of the flexible pavement sec-
TABLE 6
SUMMARY OF PERFORMANCE EQUATION RESIDUALS FOR FLEXIBLE PAVEMENTS

<table>
<thead>
<tr>
<th>Loop</th>
<th>Lane</th>
<th>Load Source of Data for Equation</th>
<th>No. of Residuals</th>
<th>Mean Absolute Residual</th>
<th>Log W Residuals</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>p Residuals</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>1</td>
<td>2</td>
<td>FR CF</td>
<td>97</td>
<td>0 39</td>
</tr>
<tr>
<td>2</td>
<td>2</td>
<td>6</td>
<td>FR CF</td>
<td>55</td>
<td>0 67</td>
</tr>
<tr>
<td>3</td>
<td>1</td>
<td>12</td>
<td>FR CF</td>
<td>37</td>
<td>0 55</td>
</tr>
<tr>
<td>3</td>
<td>2</td>
<td>24</td>
<td>FR CF</td>
<td>31</td>
<td>0 47</td>
</tr>
<tr>
<td>4</td>
<td>1</td>
<td>18</td>
<td>FR CF</td>
<td>62</td>
<td>0 66</td>
</tr>
<tr>
<td>4</td>
<td>2</td>
<td>32</td>
<td>FR CF</td>
<td>66</td>
<td>0 33</td>
</tr>
<tr>
<td>5</td>
<td>1</td>
<td>22.4</td>
<td>FR CF</td>
<td>56</td>
<td>0 64</td>
</tr>
<tr>
<td>5</td>
<td>2</td>
<td>40</td>
<td>FR CF</td>
<td>63</td>
<td>0 40</td>
</tr>
<tr>
<td>6</td>
<td>1</td>
<td>30</td>
<td>FR CF</td>
<td>77</td>
<td>0 78</td>
</tr>
<tr>
<td>6</td>
<td>2</td>
<td>48</td>
<td>FR CF</td>
<td>94</td>
<td>0 42</td>
</tr>
<tr>
<td>Over-all mean residuals</td>
<td></td>
<td></td>
<td>FR CF</td>
<td>638</td>
<td>0 54</td>
</tr>
<tr>
<td>Over-all root mean square residuals</td>
<td></td>
<td></td>
<td>FR CF</td>
<td>1,171</td>
<td>0 31</td>
</tr>
<tr>
<td>Correlation indexes</td>
<td></td>
<td></td>
<td>FR CF</td>
<td>1,171</td>
<td>0 69</td>
</tr>
</tbody>
</table>

1 Weighted applications.
2 FR = fractional replicate, values given in the order Block 1, Block 2, Block 3; CF = complete factorial.

2. Experience has shown that at the Road Test certain interactions may be considered negligible.

3. Interactions may be neglected for two other reasons: (a) Although an interaction effect may be significant statistically it may have no practical significance; and (b) interaction effects can be difficult to interpret and in the interests of simplicity and convenience they may be omitted.

4. Other Road Test data can be analyzed using only a fractional replicate of the complete factorial experiment. Experience with Road Test data has shown which dependent variables were affected by interactions and which were not. An analysis of part of the data could save considerable time and money, particularly if there were no facilities for rapid calculations with large amounts of data.

5. Regional road tests could be conducted on a block basis with the differences between blocks confounded with the differences between regions. In this way a complete factorial ex-
experiment could be designed to cover several regions and at the same time yield estimates of main effects, interacting effects and block effects with the precision of a factorial experiment. Differences between regional effects could be estimated.

REFERENCES